of a Silvered Glass Reflector, and on the Modern Reflecting Telescope and the Making and Testing of Optical Mirrors; and N. S. Shaler, Comparison of the Features of the Earth and the Moon, presented by the Smithsonian Institution; Porträtgallerie der Astronomischen Gesellschaft, presented by H. W. Tullberg; E. T. Whittaker, a Treatise on Analytical Dynamics, presented by the author.

Twenty charts of the Astrographic Chart, presented by the Royal Observatory, Greenwich; spectrograms on the rotation of the planets, &c. (six prints), presented by Percival Lowell.

On the Completion of the Solution of the Main Problem in the New Lunar Theory. By Ernest W. Brown, Sc.D., F.R.S.

The completion of a laborious piece of work which has occupied many years for its execution furnishes a suitable opportunity for giving a general account of the object for which it was undertaken and of the methods by which the results have been obtained. The problem under consideration was that of the motion of the Moon as deduced solely from the Newtonian law of gravitation. It is limited, in the first instance, to the solution of an ideal problem in which the bodies are considered as particles, and two of them move in fixed elliptic orbits round one another. This constitutes the "main problem." The history of the attempts to obtain a solution with sufficient accuracy is well known, and I shall only touch on that portion of it which is directly connected with the new theory.

The original idea of the method here adopted to obtain a complete solution—as, indeed, of nearly all the methods of those who followed—is due to Euler. The pioneer work done by him on the lunar problem has, in my opinion, never received the full credit which it deserves. This may, perhaps, be partly due to the way in which he set forth his ideas; but it is, I think, mainly owing to the fact that his work was immediately followed by that of Laplace, whose justly great reputation in every department of mathematics, and especially in celestial mechanics, has overshadowed the claims of his predecessor. However this may be, Euler fully recognised the importance of the special method under consideration. In the introduction to a paper published in 1768, "Réflexions sur la Variation de la Lune" (*Hist. Mem. Berlin Acad. Sc.* 1766, pp. 334–353), he states the problem to be considered in the paper as follows: "Déterminer le mouvement d'une Lune qui feroit ses révolutions autour de la Terre dans le plan de l'écliptique et dont l'excentricité seroit nulle, pendant

que le Soleil se mouvroit uniformément dans un cercle autour de la Terre." After some general remarks he writes : "Quelque chimérique cette question j'ose assurer que, si l'on réussissoit à en trouver une solution parfaite on ne trouveroit presque plus de difficulté pour déterminer le vrai mouvement de la Lune réelle. Cette question est donc de la dernière importance et il sera toujours bon d'en approfondir toutes les difficultés, avant qu'on en puisse espérer une solution complète." He then proceeds to find the solution, now known as the "variation orbit," as far as the fourth power of the only small parameter present. One may almost see in the few lines just quoted a germ of the magnificent work done by Poincaré on periodic orbits within the last twenty years.

The development of this idea of Euler is mainly due to G. W. Hill, who put the earlier steps into such a form that high accuracy could be obtained without excessive labour. J. C. Adams had also taken it up and worked at it in a somewhat similar manner. Hill determined the variation orbit and the principal part of the mean motion of the perigee, while Adams also found the variation orbit, but by a less powerful method, and the principal part of the mean motion of the node.

Before taking up a complete treatment from this stage it was necessary to consider as carefully as possible the amount of labour which would be demanded. The working value of a method of treatment is not really tested by the closeness with which the first or second approximation will make the further approximations converge quickly to the desired degree of accuracy ; the real test is, perhaps, the ease with which the final approximation can be obtained. Here we have the essential difference between the present method and all other methods. The approximations of the latter proceed along powers of the disturbing force. Euler's idea was to approximate along powers of the other small constants present. This gives a more rapid convergence and a degree of certainty in knowing the limits of error of the final results which no other method approaches.

With this in view it was necessary to cast the equations of motion into such a form that the degree of accuracy demanded should be capable of being obtained with a reasonable amount of labour, and it must be made clear that this degree of accuracy had actually been attained when the work was completed. Precautions against errors of computation must be taken, and the results should, if possible, be expressed in such a form that comparison with those of previous theories is possible. These and other points are considered in the following paragraphs :—

First, every coefficient in longitude, latitude, and parallax which is as great as one-hundredth of a second of arc has been computed, and is accurate—apart from possible errors of calculation—to at least this amount. Hansen, indeed, gives his results to thousandths of a second, but certain of them are in

error by some tenths of a second; indeed, it was not possible to obtain all of them more accurately without much increasing the extent of his calculations. Some of Delaunay's coefficients, owing to slow convergence, are not accurate to one second of arc. As a matter of fact my results are obtained correctly to one-thousandth of a second, and there are comparatively few coefficients greater than this quantity which have not been obtained.

Second, the theory is expanded algebraically in powers of four of the five parameters, the fifth (the ratio of the mean motions of the Sun and Moon) having its numerical value substituted at the outset. The last is known with a degree of certainty which satisfies all the possible needs of the theory, and the effect of any possible change which may be made in its observed value can be easily deduced from Delaunay's purely literal theory. The chief advantage gained is due to the fact that slow convergence (perhaps divergence) occurs only along powers of this ratio, while there is little loss of theoretical interest in using its numerical value. Moreover it is not difficult to find out how many places of decimals are necessary at the outset in order to secure a given number of places in the results.

Third, exceptional precautions have been taken to avoid the occurrence of errors during the course of the work. Equations of verification have been computed, not only at the end of large masses of calculations, but at practically every step in the process; in fact each manuscript page of work has, on the average, not less than two test equations computed. The most dangerous sources of error—the omission of a whole set of terms or the use of a wrong set—was partly guarded against by a property of the method itself. A comparatively small error of this kind produces in the final results large systematic errors, which a rough comparison with the values of Delaunay and certain properties of the solution will detect without difficulty. Very searching final tests, eleven in number, are furnished by the remarkable relations known to exist between the expressions for the mean motions of the perigee and node and the constant term of the parallax. These were all completely satisfied. Finally, the work was so arranged that computers could be engaged to do considerable portions of it. Only one, Mr. I. I. Sterner, has actually been employed, but this disadvantage was counterbalanced by the very high accuracy of his work. A rough calculation of the chances of an error slipping by both of us in the work turned over to him and verified by me and through the special test equations for it gave two or three possible errors in the whole. Two such errors were actually detected by the numerous final tests, and these were, of course, traced down and corrected.

Fourth, the results originally computed for the rectangular coordinates of the Moon have been transformed to polar coordinates, and thus a direct comparison with those of Delaunay has been rendered possible. Newcomb had previously transformed

Hansen's results to the same system, so that these are also available for comparison. This comparison will appear in a following number of the *Monthly Notices.* Nearly all the differences Delaunay–Brown can be explained by slow convergence of the Delaunay series, and in most of the remaining cases the differences Hansen–Brown are very small. Unexplained disagreements between the new results and those of both the earlier theories only occurred in the cases of coefficients difficult to determine accurately by the latter methods, owing to the occurrence of very small divisors and the slowness of approximation proceeding along powers of the disturbing force.

Fifth, comparison of the new coefficients with those deduced from observation has at present only been possible to a limited extent, but in two cases—the mean movements of the perigee and node—it has been completed. The net result is very satisfactory. The differences in the annual motions of these two lines are less than three-tenths of a second of arc, and these are capable of being explained by reasonable suppositions concerning the figures of the Earth and Moon, the constants connected with these bodies not being yet known with sufficient accuracy. One of the most important coefficients—that of the principal parallactic inequality in longitude—appears to furnish a value for the solar parallax very near the mean of all the values obtained by other methods.

A few brief details about the amount of time and labour expended may not be uninteresting. From 1890 to 1895 certain classes of inequalities were calculated, but the work was only begun on a systematic plan, which involved a fresh computation of all inequalities previously found, at the beginning of 1896. Mr. Sterner began work for me in the autumn of 1897 and finished it in the spring of the present year,* though neither of us was by any means continuously engaged in calculation during that period. He spent on it, according to a carefully kept record, nearly three thousand hours, and I estimate my share as some five or six thousand hours, so that the calculations have probably occupied altogether about eight or nine thousand hours. There were about 13,000 multiplications of series made, containing some 400,000 separate products; the whole of the work required the writing of between four and five millions of digits and *plus* and *minus* signs.

Although the problem now completed constitutes by far the longer part of the whole, much remains to be done before it is advisable to proceed to the construction of tables. The problem solved is that of the Moon under the attractions of the Earth and Sun, the centre of mass of the Earth and Moon being supposed to move in a fixed elliptic orbit. There remains to be found the effect of the figures of the bodies—mainly that of the Earth—the effects of the differences of the actual motion of the

* Most of the expense has been met by grants from the Government Grant Committee of the Royal Society.

centre of mass of the Earth and Moon from fixed elliptic motion, due to the attractions of the planets ; and, the most difficult of all, the effects of the direct attractions of the planets on the Moon. There are many periodic coefficients, due to the last, larger than one-tenth of a second of arc in magnitude, and the whole subject needs a careful and extended investigation. An attempt to complete the problem by considering anew these remaining sources of disturbance has been already started. The difficulties presented appear to arise much less from intricate calculations than in the construction of a satisfactory method which will give the assurance that no sensible terms have been neglected. If a moderate degree of success attends these efforts it should be possible to discover whether, within the limits set by the observations, the motion of the Moon shows effects which cannot be traced to the direct operation of the Newtonian law of gravitation.

Haverford College: 1904 *November* 18.

Analysis of 145 *Terms in the Moon's Longitude,* 1750–1901. By P. H. Cowell.

In this paper I give an analysis for 145 terms in the Moon's longitude. The observations used are the Greenwich meridian observations from 1847 to 1901 ; and in the case of all terms whose period exceeds two months, or whose period is equal or nearly equal to the anomalistic month or a sub-multiple of it, the analysis is extended so as to embrace the period 1750 to 1851 as well.

I have used the lunar day as the unit of time. The advantage of doing this is explained in a previous paper (vol. lxiv. May).

For the short-period analyses I have adopted auxiliary angles which complete an exact number of revolutions in an exact number of lunar days. The advantage of doing this is explained at the end of the paper above referred to. Owing, however, to the absence of observations near new moon it is not possible to adopt an auxiliary angle instead of the mean elongation D ; during a single period of analysis (400 lunar days) I have treated D as going through two revolutions in 57 lunar days, but the error has not been allowed to accumulate from period to period (vol. lxiv. December). This distinction is rendered necessary, because the analysis for sin D is not similar to that for cos D, whereas that for sin ${}_mA_n$ is similar to that for cos ${}_mA_n$, the angle ${}_mA_n$ being defined as completing n revolutions in m days.